Sally Benson's ESD Story

I came to the Lab in 1977, which was of course right in the middle of the energy crisis. I had just graduated from college and had come back here to be with my family. My mother found out about a job at the Lab and I applied for it, met with two people—Nari Narasimhan and Ron Schroeder—told them what I had done at school, that I'd gotten my geology degree. The next thing I knew they hired me (that was wonderful!).

I was brought in to work in the geothermal program (at that time it was a very field-oriented program), specifically on these very big field experiments (one of them at East Mesa, which went on for many years, the interference test at that site). We had state of the art instrumentation, highest resolution, all very exciting stuff to work with.

My job initially was to take all the data we collected from the field and get it put into the computer. I would walk these grocery tapes of data to this big key punch operation in Bldg 50 (part of the 7600 computer, at the time the leading supercomputer in the world), give it to the people there, and they would punch them all in, and they would give me these cards back. Anyway, I would read these huge decks of cards into the computer, got pretty good at it, got the computer to make these graphs automatically. It was really fun, basically I could do what I wanted. I worked all the time—there were always a bunch of us in the computer center, running all these programs—and I was learning a huge amount. Part of loving the Lab was just the ability to learn with all these really smart, energetic people and having these incredible computational resources and experimental resources, and having people encourage you to expand your abilities. I was always looking for more to do, and luckily there was always somebody there saying "Sure, here's more to do," and with this new work thrown my way I would learn more. I wasn't being restricted to "my job," since my job was getting constantly redefined. I never got bored.

I started getting further involved in field projects, and again lucky for me, we had the world's pre-eminent field crew—pressure measurements, borehole measurements, and geophysics (a group that included Ray Solbau and Don Lippert)—world-class, creative, inventive, and handy, all wrapped up into one. The additional allure was the travel that this work required—I went to Mexico (Sierra Prieto), East Mesa, the Imperial Valley. I also started working on papers and publishing them, again getting all the encouragement in the world to do that. Very exciting times here.

So that was the early days, 1977–78, and then during that time, we also got involved in low-temperature geothermal energy resources, which are used in direct heating. (As opposed to using electricity, you would pump in hot water and heat your home or whole city blocks with that source.) I started working on a small geothermal project in Klamath Falls, Oregon. We did this experiment that took the whole summer of 1978. I moved up to Klamath Falls. It was my first experience in working closely with the local community, a necessity on this project because the city had gotten funding to do a big project, to heat the whole downtown with geothermal sources, including some low-income housing. However, the rich people in town (on the hill) didn't want this geothermal resource to be

developed because they thought the water levels in their wells would go down. My work there was to investigate whether geothermal heating of downtown would have this adverse affect on the elevated folks. Now, there are a lot of retired people in Klamath Falls, and our moving up there was quite a curiosity for them: These people were fascinated with the work we were doing. They all knew where I lived, and I would leave the door open. When I'd wake up in the morning, I'd come out to breakfast and there'd be all these old guys in my living room, looking at all the data collection charts! They were fascinated. Not only that, but every week I would go on the local TV station to talk about how we were doing, the progress of the experiment and so forth.

I think that, in addition to all the encouragement I got from the Earth SciencesDivision, this experience got me interested in working on problems that were not just purely scientific, but which had a human and social dimension. These problems can take surprising turns. At one point, the head of Earth Sciences Division, Paul Witherspoon called me in to his office and asked me what I was doing "up there." Apparently the Lab director had gotten a letter from Ernest O. Lawrence's brother, John Lawrence, who happened to live in Klamath Falls, saying "Benson was doing this and Benson was doing that!" I don't think he knew too much about the experiment, only that it was controversial. So I explained to Paul what was going on "up there" and showed him the data. While I'm sure he didn't like getting a "what's going on" note from the Lab director, Paul gave me a tremendous amount of support after hearing me out. It was another step for me, I think, and gave me confidence to move forward with my career, in what continued to be a very supportive environment.

But then in the early 1980s, everything changed for ESD. We had started out with this geothermal emphasis, but under the Reagan Administration, the geothermal-energy-related work was cut back drastically. A lot of people left (some by choice and some not by choice) and the atmosphere changed. This forced a broadening of the division's work to include nuclear waste storage. Paul Witherspoon and his group had begun work on the Stripa project in Sweden, and this work continued into the 80s. We had also started similar work at Hanford up in Washington.

About that time, Tom McEvily came into the Division and he brought a very strong interest in seismology, so that was another big thrust that started at that time, the development of CCS – the Computational Seismology Center. Don DePaolo came in a little later. At that time scientifically there were two core strengths of ESD, hydrogeology and geophysics, both electrical geophysics like what Frank Morrison does and also seismology. Those were the scientific parts of the Division.

So there we were in the 80s, with significant shifts in the funding situation and some new leadership. I continued doing geothermal work. At that time, I liked what I was doing at ESD, but I decided that I needed to get a graduate degree if I was ever really going to have the career I wanted to have in science. So I went to graduate school at U.C. Berkeley while I continued to work in ESD almost full-time, and would eventually get degrees in material science and mineral engineering. Nari was my advisor.

Paul Witherspoon was really wonderful—he attracted students and scientific talent from around the world. There was a big infusion of U.C. graduate students at that time. Even after he was no longer Division Director, Paul was just always somebody who recognized talent and encouraged talent—Iraj Javandel, Marcelo Lippmann, Nari— so many people, that Paul was very important to.

Then come along the mid-80s, I was writing my PhD. thesis but working full time, because I had finished my coursework. I was sitting one night watching 60 Minutes on television and there was a really interesting show on called "Further Down the Drain," about selenium contamination in the Central Valley of California. And it was as 60 Minutes does it, a very compelling story. There were awful pictures of the dead birds, pictures of the farmers, and the government, scientists. I thought this was really fascinating. About a month later, a group of people here, led by Oleh Weres and Art White, two chemists, became very interested in selenium and selenium chemistry, so we formed an interdisciplinary team of hydrologists, chemists, limnologists from the Lab and the campus, using the fundamental idea that the geochemistry of selenium was such that you could immobilize it and detoxify it as long as you kept it wet. So the team went up to the Bureau of Reclamation in Sacramento and basically told them that we think we've got an idea about how you might be able to solve your problem. It took lots and lots of discussions, but eventually they said okay, come and help us.

I had gone now from being basically a deep subsurface geoscientist to working in a very shallow environment. We had this big mish-mash of geochemists, ecologists, it was really my first opportunity to work with a team of people where you needed their knowledge but it was really very different disciplines. I think in many ways that was some of the early interest in the highly interdisciplinary work here and really around the world in the environmental sciences. They started to deal with all these elements at the same time.

I worked on Kesterson along with Tetsu Tokunaga, Peter Zawislansky—they were also important parts of that. And we worked between 1985 and1989 to develop the scientific knowledge and underpinnings that allowed the government to make a decision about the remedy. We worked closely with the state and water quality control board, and that was another really fun thing. We were doing cutting-edge science, this was the intersection between biology, geochemistry, and hydrology, with the bits and pieces still not very well developed—we were doing this cutting-edge science, and at the same time decision-makers were needing what we had to offer, to figure out how to remedy the problem. So for me this was the kind of thing I really enjoy: great science, but also great impact, with the ability to inform and help guide decision-making.

Similar to but to a much greater degree than the work in Klamath Falls, I was getting a policy perspective about environmental issues. The Kesterson project was a big educational period here for me and also was really, I believe, the first truly environmental work that ESD did. Iraj came back just about that time, it might have been a little bit earlier, and he and Chin-Fu Tsang and Chris Doughty and Paul to some degree were also

doing more theoretical work in groundwater cleanup and so forth. And also becoming interested in the site problems, which really happened on a larger scale somewhat later.

That became the next big thrust of the Division: the environmental remediation work. So fresh off what was viewed as success in Kesterson, I had become very bullish on the idea that scientists could really make a difference, with science driving good policymaking, saving money, saving birds, the whole thing.

At that time, there were also administration changes. This was 88–89, with the beginning of the first Bush Adminstration. Around this time DOE was finally really coming to grips with the fact that its sites were a disaster, that most of the nuclear weapons production sites had serious contamination problems. So they began to grow a program. I thought that I knew a lot, knew the science, kind of knew how to bring together the pieces. I started working very closely with DOE to try to build a program that would provide the scientific underpinnings to support all their cleanup work. I became involved with groups like the Strategic Lab Council, a group of senior people from most of the labs—I wasn't the most junior person by far, but I was perhaps one of the more scientifically oriented ones. We were able to help develop some very important research programs—the Environmental Management Science Program, which continues on today, and the NABIR program—they were done in parallel—two big new research programs in environmental sciences, both of them highly interdisciplinary. I was doing it both because I wanted to help DOE and because I thought that it could be generally helpful in trying to create opportunities for people here. In fact we were able to create funding opportunities and then started recruiting in the area, brought in people like Terry Hazen and so forth (that was a little bit later), got them here. Jenny Hunter-Cevera came here for that, too.

Then I became Division Director, I think in 1993, but I started as Acting Director in 1992 when Tom McEvilly decided to step down. That was also about the time that Chuck Shank became Lab Director, around 1989 or 1990. During that time frame, some big changes happened. We became more involved in nuclear waste storage work through the Yucca Mountain Project. Initially, we were doing support work on YMP for other labs. We had been supporting Hanford as the national nuclear waste storage site and had done a little bit of work related to prospective salt sites, but we weren't in anything more than a support role. Sandia had YMP work, Los Alamos, Livermore, and we would do a little bit of modeling work for them—we weren't really on the team.

When Yucca Mountain was chosen as the sole site to be investigated, we tried to organize ourselves to play an effective role there. Now, soon after I became Division Director in1993, I went to a seminar in which some of our people gave talks that were apparently contradictory. This was certainly not impossible or unprecedented in genuine scientific discourse, but in this case I felt that the real problem was simply that we were all working for other people, other labs, and that we weren't having the internal discussions that would have perhaps allowed us to truly sort out all the possibilities ahead of time.

That's when I asked Bo to lead the nuclear waste storage program. And he proceeded to do an amazing job—he took these disparate pieces and eventually persuaded OCRWM,

in mid-1993, to make us one of the formal team members. That was a huge accomplishment, to get our name on the list recognized as a major contributor. And in particular all the vadose zone studies were the niche that had people really looking to us, because we had all this TOUGH2 (?) modeling capability and the multi-interacting continua that allowed you to have both fractures and blocks at the same time.

The other thing we didn't have with respect to being part of the DOE nuclear waste storage group was a QA program—you couldn't really be a player in this program unless you had one. So Bo very smartly said, well, let's have a QA program! We actually used internal funds to build that up, which then allowed us to become a major participant. Through the years, the role of ESD in the YMP continued to grow; with the heater tests especially, we were able to carve out a major role for ourselves in the field—now with ambient testing, for the first time in Yucca Mountain. So that was very exciting. Then we got more involved in some of the saturated zone studies, and basically the influence of our program grew as our work's relevance, quality, and impact warranted.

But as far as Bo's drive and ability to communicate, I think what made him so successful was that he had as a goal to solve the Yucca Mountain nuclear waste storage problem, to find out conclusively whether the site was feasible as a storage site. That's what drove him: How to take the natural system, the site itself, together with engineered enhancements, and make it work. It wasn't "Oh, we want to build a code," or "Oh, by the way, this is an opportunity to use the code." He was 100% in sync with the leadership of DOE, and with the private sector site managers, to solve the problem—and they knew that. And he could bring together this really strong team.

Anyway, nuclear waste started doing really well. We also had (and have) an energy program that we very much wanted to work with the private sector on imaging and EOR and basically to build up something there. That was kind of struggling because at that time there was less interest on the part of government to fund that kind of work; they felt that was a private sector problem. The environmental work was growing very well, Yucca Mountain work was growing very well.

Our fundamental core sciences program – the BES program—was also quite stable. What we were trying to do in this program was to look at our applied, mission-driven programs, what the DOE is here to do, and how our division could support those missions. So we would look at the underpinning science, determine what the core problems were, look at the fundamental scientific issues in common, and those were the areas where we would try to write proposals and be persuasive. So very much in the 90's there was the vision, this model, for how the Division should work. That we were there to support DOE, and we would draw a chain all the way back from these applications to fundamental science. And to provide real value added by transfer of knowledge between these different application areas. So, for example, in environmental sciences, they are generally dealing with issues on a smaller, local scale, and they're typically closer to the surface, so many of the things we do in that work are at a much higher resolution than for an oil and gas reservoir problem. As we would learn some innovation, or how to do something in a very high-resolution way, we would transfer that over to oil and gas.

In this way there was this continual sort of churn that operated, not so much in the fundamental science area, but sort of at the mid point, just sort of figuring out how we could borrow from one field to apply to another. I think that is part of what has made us so successful. There is this continuous source of innovation, drawing from these various application areas that allow us to continue to find solutions. If you look at many academic settings, that really isn't going to happen. People end up being channeled or pigeonholed in some ways: "Oh I am in a petroleum engineering school, therefore I do things this way and that's all I know." By having it all in one big place with everybody talking and having seminars, we kind of overcame that.

That gets us to the mid-1990s. Things were good, the Division was growing. And as the Division Director, your job is to figure out how you can keep it all going, what's the next arena that will be important, and that we can make a difference in. I realized we didn't really do anything with respect to climate change. I started getting interested in the climate problem, spent some time trying to learn and read and so forth. Luckily, about that time, the DOE under Martha Krebs decided to do a study to look at fundamental research needs in the area of climate recovery management. So I was lucky to be asked to work on that team, we surveyed the waterfront, and found two main areas that earth sciences could play a role in. One was terrestrial sequestration—the kinds of things that Margaret Torn works on—and the other one was CO_2 sequestration in deep geological formations.

Those seemed like promising areas. After Martha Krebs did her own assessment, she formed a partnership with the Office of Fossil Energy. That was Martha's genius, to really build trust in working with these other application areas within DOE and thereby design research programs of mutual benefit. So this new project was created to establish a carbon sequestration road map, and we specifically decided that of all the areas that had been studied in carbon management, sequestration was the one in which there was really almost nothing done, it was brand new.

In light of that, we worked for about nine months or so with Fossil Energy and Office of Science to develop this road map—which we did. That then became the basis of a rapidly growing program in carbon sequestration in the Office of Fossil Energy. We then put in a proposal for something called the GOC project, which was intended to be a large, multi-investigator, multidisciplinary grant in the area of geological storage of CO_2 and we won. We were very delighted because it was a big program— $2 million a year –a very sizable program, you can get a lot done with that. So that was the seed of that program. Since then, we have been tremendously successful. We have a large program, more that $5 million a year, working on geologic sequestration of CO_2.

So rolling back the tape a little bit, when the issue came up what should our carbon program or climate change program be, it seemed to me that there were several important things. One was geologic storage, another was terrestrial sequestration— planting forests, managing soils, understanding the stability of carbon pools on the near surface. That's when we recruited Margaret to come in. We had no idea whether we could get any

funding in this area whatsoever, but we had confidence in Margaret's capabilities. Fortunately, at that time something called the ARM program, the Atmospheric Radiation Measurements program, needed someone to lead the efforts to add CO_2 monitoring into the ARM program Margaret wrote a successful grant—a large grant, particularly for a young PI, and anyway she has done a wonderful job with that and the program has grown too. That's the second pillar of the program.

And the last pillar in the Climate Program was regional climate modeling, because at the time we didn't have any global climate modeling capability here. I looked around and saw that there were a lot of other people doing that. It started me wondering, how could we make a niche for ourselves? I thought the answer was to focus on the smaller scale, regional climate, specifically California, and tackle the "what's going to happen to us, here," question. So we brought in Norm Miller and his program, took advantage of his skills, his very-high-resolution models that look at precipitation and temperature, with emphasis on the West, particularly the water resources in California. At the same time, (around 1998) the U.C. Berkeley campus was building up its climate change program and had hired Inez Fung to lead their effort in climate change. And she being married Jim Bishop, we were very fortunate to get him to work on ocean carbon cycling, so that became another exciting part of the program.

Then, of course, Bo came along as Director when I left ESD in 2001 and did a lot for the division's organization, laying out clear expectations for people in terms of productivity. He himself tried, successfully I think, to move from being "the tough guy" to making sure that everyone felt like part of the same team, and successfully building up all the ESD programs.

Now we just have to see what comes next. This recent biofuels effort, I would certainly hope to see that ESD could play a strong role in that. It is after all, earth processes that are involved in growing the fuel.

I guess, looking back, when I first came here, it seemed—and maybe it was because I was young that it seemed this way, or maybe it was true—that there were more opportunities, funding wasn't so hard to get; there were more large programs instead of individual PI funding. My sense now is that worry about funding has become constant, and it's something that's difficult for people to deal with. On the other hand, we're certainly a bigger, stronger, more diverse organization than we were then—I mean, this place is a powerhouse of intellect! I would easily say that, collectively, the ESD is the best geoscience program in the world, if you look at it in its entirety, and you compare it to any other organization collectively.

Also, I sense that people like to and expect to work together more now. There wasn't the case when I first came here—the sense of camaraderie. I think that's grown over the years. Perhaps with the complexity of the problems being encountered, people know they can't solve the problems they're working on all by themselves, and that they really need experts in other disciplines to help. You can only know so much. People are educated and trained in a more interdisciplinary way now, yes, but my sense is that to really do the

cutting edge work, you have to work collaboratively with the highest expertise, call on experts outside your field. I think ESD really does that well now.

That may be attributable to a group maturation process, too. When I was first here, it was a brand-new, maverick kind of place. Virtually nobody had been here for 30 years. Now we have those sorts of veterans in ESD. But even those people who have been here for all those years, say someone like Karsten, you still see that they have this amazing energy and productivity and impact. It's important, and wonderful, that this sort of energy has never left.